Reviewer’s report

Title: Does academic interest play a more important role in medical sciences than in other disciplines? A nationwide cross-sectional study in China

Version: 1 Date: 02 Jul 2019

Reviewer: Dirk Tempelaar

Reviewer's report:

I appreciate the attempts by the authors to include the feedback of editor and reviewers in the revised version of the paper. From my perspective, most issues are adequately covered, with only two issues I would appreciate some further revisions.

The first relates my comment2: the use of 'causal language'. Although some PSM studies claim that 'PSM allows causal inference in observational studies', that claim is not without debate. The claim being valid is fully depending on the assumption that there are no unobserved confounders. But that assumption cannot be tested. In fact, the Zheng paper does not even mention this assumption: it discusses three main assumptions at the bottom of p. 10, top of p. 11, but not this one. It is not difficult to bring forward arguments why the assumption of no unobserved cofounders is unlikely to satisfy. The 'treatment' is no treatment in the true meaning of that term, but the presence or absence of interest, an observed quality. We know from an abundance of empirical educational research, that having interest generally goes together with many different adaptive learning dispositions, such as being engaged, motivated, having proper learning goals, and so on. All of these learning dispositions are potential cofounders. But if so, all are unobserved, because the covariates exclude any dispositional student variables.

Why insist so strongly on this causal language? If you are slightly more modest in your claims, do not talk about 'causal effects' but just about 'effects', you do not hurt those researchers who are more strict, and at the same time can bring your message?

Somewhat related is my second issue, related to my comment1. The author has introduced a new reference to justify some analytic choices: Caliendo M, Kopeinig S. Some Practical Guidance for the Implementation of Propensity Score Matching[J]. Journal of Economic Surveys, 2008, 22(1): 31-72. First of all: I cannot find the ATT formula in that paper as it is given in the Zheng paper. Second, and more important: in the discussion of the ATT criterion the referred paper mentions the importance of the 'socalled self-selection bias', and continues to state: "In social experiments where assignment to treatment is random this is ensured and the treatment effect is identified. In non-experimental studies one has to invoke some identifying assumptions to solve the section problem stated in equation (3)." That provides a nice summary of the issue in the Zheng paper: the treatment is not random, since it is an observed learning disposition, implying that these identifying assumptions are of crucial importance.

Are the methods appropriate and well described? If not, please specify what is required in your comments to the authors.
Yes

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

Yes

**Are the conclusions drawn adequately supported by the data shown?**
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Yes

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